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~~CENTRAL PUBLIC HEALTH LABORATORY~~, Bureau,
COLINDALE AVENUE,
LONDON, N.W.9.

7th October, 1953.

Dear Dr. Lederberg,

Thank you for your letter of the 11th September which I found on my return from the Rome Congress.

I was very glad to learn that the paratyphoid-B typing phage B.A.O.R. and the typhoid phage little k proved to be useful in transduction experiments. From the rather scanty information passed on by Dr. Anderson it appears that you have transduced the typhoid H antigen d to a Salm.paratyphi B strain, by means of a lysate of Salm.typhi prepared with phage k.

I do not quite understand the meaning of what you wrote about the negative results with phages O-1,-2,-3. On the one hand, you wrote that the stock preparation of phage k readily transduces motility to O 901; on the other hand, you stated that the phages O-1,-2,-3 "give negative results in transduction assays, even with O 901 lysogenic for k as indicator". From the first statement I would assume that when O 901 becomes lysogenic for k it also becomes motile, but apparently this assumption is not correct.

Dr. Anderson has now sent you the full set of the available latent phages of Salm.typhi, and I hope that more of these will prove useful in your experiments. In this connection I would like to draw your attention to the fact that these type-determining phages have been first described in Nature, 167, 603, 1951, not in the paper you have quoted on page 420 of your paper in Physiological Reviews, 32, 403, 1952 (your reference No.3). In the set of ten paratyphoid-B typing phages, which you received from Chamblee, the following are known to be type-determining phages: 3b, Beccles, Taunton, B.A.O.R. and Dundee (see Table V in Lancet, 2, 10, 1951; and Nicolle et al., Ann.Inst.Past., 80, 496 and 479, 1951).

You will remember the earlier correspondence about Professor Crézé of Angers, and you will be interested to hear that what he claimed was well founded. I examined a number of the cultures he used in his experiments and there can be little doubt that he succeeded in inducing Vi-antigen formation in authentic cultures of H 901 and O 901 which I sent him. In fact, these cultures were from the same batch as those I sent you on the 10th February, 1953. The two interesting points in Crézé's results are:

(a) In each instance the change was produced via the typically 'rough' variant. (Please look up the penultimate paragraph in my letter to you dated 1st January, 1953).
(b) The conditions of the experiments appear to preclude participation of active bacteriophage.

I understand Professor Crézé now has a preliminary note in the press.

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Dr. Anderson did not tell me that you would like to have some of my old reprints. A few days ago I sent you two envelopes containing a collection of old reprints. Some of the ancient papers are now out of print. If some of the reprints sent are of no interest to you they may perhaps be of some use to one of your co-workers.

With kind regards,

Yours sincerely,

A. Fuj

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